ISSN 0029-3865

CBPF-NF-050/84

THE EARLY PERIOD OF THE UNIVERSAL FERMI INTERACTION*

by

Jayme Tiomno

Centro Brasileiro de Pesquisas Físicas - CNPq/CBPF
Rua Dr. Xavier Sigaud, 150
22290 - Rio de Janeiro, RJ - Brasil

* To be published in the Proceedings of the Conference on Fifty Years of Weak Interactions: from the Fermi Theory to the W, Wingspread, Wisconsin, May 29-June 1, 1984, edited by American Institute of Physics.

ABSTRACT

A critical analysis of the contributions which lead, in the early period, to the discovery of the universality of Fermi-type weak interactions is made. In particular the current references to this universality as "Puppi's triangle" are shown to be incorrect.

Key-words: Universal Fermi Interaction; β-decay; β and μ-capture; μ-decay.

I shall make a short review of the early period of the universal Fermi interaction, partially covered in Marshak's talk, including my own contribution as a physicist of the third world. I must mention that I was startled, not to say shocked, at the 1982 Paris Conference on the History of Particle Physics (1930-1960). I discovered a new version of that period, different from my own recollection on which I shall report here. I call this "The Case of the Mutating Triangle", with the "Tiomno-Wheeler triangle" evolving in time into "Puppi-Tiomno-Wheeler's" to finally become only "Puppi Triangle".

After the second world war, I started to study in Rio de Janeiro, and then in São Paulo, Cosmic Rays and Particle Physics. I like to mention a seminar during the second semester of 1947, given by Lattes, a younger colleague of ours who had participated actively in the discovery of the π-mesons in the $\pi \to \mu + \mu_o$ decay in Bristol (with $m_{\mu_o} \sim 100 m_e$). We were excited since the puzzle resulting from the Conversi-Pancini-Piccioni experiment would be solved if the π was the meson involved in nuclear forces, say, the Yukawa meson. But why did the μ-meson have a weaker interaction with nucleons? I proposed then that μ, μ_o were not Yukawa mesons, but spin-1/2 particles forming an isodoublet. The μ-capture $\mu^- + p \to n + \mu_o$ did not have then to go fast. It was objected that the cosmic-rays mesons decayed as $\mu \to e + \nu$ and not $\mu \to \mu_o + e + \nu$ as I was suggesting. The reason I mention this will become clear immediately -it is not to claim priority. I use here the old notation

($\mu_o \equiv \nu_\mu$ and $\nu \equiv \nu_e$). I must mention that I was not aware, at the time, of the Bethe-Marshak paper (Phys.Rev. 72, 506, 1947) on the two meson hypotesis which preceded π discovery.

I had been already accepted as a graduate student in Princeton to work with John A. Wheeler starting January 1948. So I stopped thinking about this problem and, at Princeton, I became involved with graduate courses and in general relativity research with Wheeler.

By the beginning of June, Wheeler gave a talk in a seminar which was a preview of the presentation of the papers submitted to the Pasadena meeting (June 21-23). I was very excited when he told of his results on the spectrum of the 3-body μ-decay ($\mu \rightarrow \mu_o + e + \nu$) obtained from pure phase space computation, assuming a Fermi type interaction. I think that preliminary results had already been mentioned by Wheeler at Pocono meeting (April, 1948). He found that the coupling constant was approximately equal to the Fermi coupling in β-decay. In another paper, he obtained negative results of μ-nuclear capture calculated as a second order effect from μ-decay followed by e-ν nuclear capture, both by Fermi interaction. The μ-capture rate obtained was extremely small.

I was excited because I realized that I could have done the first calculation in Brazil. Also because I thought that the μ-capture could be the analogue of the electron K-capture by Fermi interaction and I then obtained, in a crude approximation, that again this Fermi coupling was nearly the same as in β-decay. Wheeler checked these results and we then engaged in a collaboration to study further both processes. We decided that for μ-decay, we should introduce the dynamics of the Fermi in teraction via the several simple types of possible Fermi couplings of four spin-1/2 particles. This might lead, by comparison with experiments in μ and β-decay, to the discovery of the correct unified Fermi theory. We should consider several possible values of the μ_o-mass, down to zero (both for $\mu_o \neq \nu$ and for $\mu_o \equiv \nu$). We should also work out more detailed models for μ-capture to make stronger the case for the unification of these processes by Fermi interactions and also to predict the amount of nuclear excitation and stars produced. Indeed we were not satisfied with only the simple coincidence of the three coupling constants. We started to work immediately.

Wheeler went to Pasadena (Caltech) to report on the new state of the two problems. Then we worked extremely hard for the next two months to complete the calculations of our extended program, which were included in the Proceedings of the Caltech conference. At the Centennial Meeting of the American Association for Advancement of Science in Washington, D.C. (September 15, 1948), not only the triangle relation but also other results of these papers were presented in Wheeler's invited talk. The papers were written but now, instead of the proceedings, the Pasadena papers were to be published in an issue of the Review of Modern Physics. After some delay, the January 2, 1949 issue was dedicated only to the Symposium. There, below the Table of Contents, it is explicitly stated that all papers of that issue were presented at the Caltech Symposium and that the manuscripts were edited and extended by the authors.

Ours were really two papers: "Energy spectrum of electrons from meson decay" (pages 144-152) and "Charge exchange reactions of the μ-mesons with the nucleus" (pages 153-165). The μ-decay paper had a detailed analysis, as indicated above, stopping short of the consideration of mixed Fermi couplings. The comparison of the three Fermi processes and the triangle visualizing the near coincidence of the three couplings as well as the observation that this indicates a close relationship of the three reactions, are made in the second paper. In both papers, we used the doublets (μ, μ_o), (e, ν), with the implicit lepton number conservation. For massless μ_o's we considered $\mu_o \neq \nu$ (two neutrinos theory) as well as $\mu_o \equiv \nu$ (one neutrino theory). The treatment was very detailed and relativistic wave functions were used for leptons.

Some of the confusion in connection with the issue of the universal Fermi Interaction may have resulted from the fact that a number of papers refer only to the first of our papers in RMP (even the Yang-Tiomno paper !!! Also in Marshak's review referred to below). In the μ- capture paper, we added a note in proof mentioning a preprint of the "Letter to the Editor" of Lee, Rosenbluth and Yang (Phys. Rev. 75, 905, 1949 received Jan. 3, 1949). They obtain near equal couplings (with $\mu_o \equiv \nu$), also explicitly assuming Fermi-type interaction for all these processes. Besides, they foresaw the possibility of the intermediate Boson.

As our papers give no receiving date, it should be helpful to mention that in the paper on μ-decay, footnote (2a), added in proof, refers to a paper of Horowitz et al. (Phys. Rev. 74, 713, Oct. 1, 1948) "which appeared after the present article had been submitted for publication".

These two papers of ours had much repercussion and soon the expression "Tiomno-Wheeler triangle" started to be used. I thought that this discrimination was unfair to the paper of Lee-Rosenbluth-Yang, if only because it was published one month later. I was then convinced by a number of people that it was not unfair because many physicists knew of our results since Pasadena and Washington and also because our papers were more complete and had a number of results which could be, and were, compared with experiment. My idea was to examine in my PhD thesis the general combination of interactions to try to determine the correct combination, as β-decay could not be adjusted to any single coupling. (Notice that I am leaving the papers of Pontecorvo, Klein and Puppi for later). The fact that Wheeler left Princeton for one year and that I decided to do my PhD thesis with Wigner (on neutrino theories and double β-decay), suppressed this program which was brilliantly performed by Michel (Proc. Phys. Soc. A63, 514, 1371,1950). This last paper did not lead, however, to the final correct Fermi interaction but introduced the famous Michel parameter.

The first attempt, to my knowledge, of finding the form of the Fermi unified interaction by symmetry considerations was made by Yang and myself (Phys. Rev. 79, 495, 1950) in a paper which coined the name "Universal Fermi Interaction". We used the Wigner-Critchfield S-A-P interaction which is symmetrical in the four fermions, but this did not stand.

During the next few years, back to Brazil and out of the main flow of ideas and information, I proceeded in the attempt to select a theory by symmetry arguments. Indeed, I published a paper (Nuovo Cim. 1, 226, 1955) on the γ_5-transformation which I had introduced in my PhD Thesis. It had been rediscovered by

Peaslee (Phys.Rev., 91, 1447, 1953) for massless particles as: $\psi_\nu \rightarrow \gamma_5\psi_\nu$. For massive particles I had, as in my Thesis (Princeton, 1950): $\psi_a \rightarrow \pm\gamma_5\psi_a$, $m_a \rightarrow -m_a$ and called it now, after Peaslee "mass reversal invariance". Besides other consequences, this invariance principle eliminated Fierz interferences. I added it, as a further condition, to the symmetry conditions on a pair of members of the 4-fermion interaction (proposed by Pursey: Physics 18, 1017, 1952). With this I reduced the three Pursey's possibilities for the UFI to only two: S+P-T or A-V (with parity conservation) for the usual ordering of β-decay. The mass reversal invariance was another instance of mutation as, after being used in several papers (e.g., Abdus Salam, "On Fermi Interactions", Feb. 1957 (unpublished); J.J. Sakurai, "Mass Reversal and Weak Interactions", Nuovo Cim. 7, 649,1958), it changed into chiral invariance.

Finally, after the discovery of parity violation, (Nuovo Cim. 6, 1, 1957 - received July 2), using the 1/2 $(1\pm\gamma_5)$ neutrino projection operators that I had introduced in the first draft of my Princeton PhD thesis (1950, unpublished), I have shown that both S+P-T and A-V, with appropriate helicities of the ν's were in agreement with all observations except β-decay which, according to Mahmoud and Konopinski (Phys.Rev. 88, 1266, 1952), gave $\alpha S + \beta P + T$ with $|\alpha| \sim 1$, $|\beta| \lesssim 1$. Thus I chose S+P-T as the UFI.

When I told Feynman of this paper during his stay at the Centro Brasileiro de Pesquisas Físicas in Rio de Janeiro (1957), he said that he was working in an Universal Fermi Interaction with $(1-\gamma_5)$ projectors, using his second order version of "Dirac"

Equation, and that the experimental results were changing, so that S+P-T was already excluded. I remember his calls to Caltech, every Monday, by amateur radio to check results and to get new experimental data on β-decay. All this, I concluded later, had to do with the forthcoming paper with Gell-Mann on the V-A theory, already covered in Marshak's talk. I must say that I allways preferred Marshak-Sudarshan approach for obtention of the V-A theory, using $(1-\gamma_5)$ projectors with the ordinary Dirac equation, at the Venice Conference (September, 1957).

Now I come to:

The Case of the Mutating Triangle

First I mention that our first paper in the Rev. Mod.Phys. acknowledges O.Klein for the proposal that μ-decay was an ordinary β-decay, showing that with Fermi coupling, the lifetime of μ resulted of the correct order, although Wheeler had this result in May or before. However, we only discovered Klein's paper at the time we were writing our papers by September 1948. Indeed, if one compares the quotation atributed to Klein on page 144 of the μ-decay article in R.M.P. with Klein's paper, one sees no direct relation. Indeed, the expressions "Klein has noted" and "Klein points out" were included after the first draft. I knew of the Puppi paper only after 1950 when I was back in Brazil. This was the time when some authors started to refer to the "Puppi-Tiomno-Wheeler triangle".

After the Paris Conference (1982), when I knew that now it had become just "Puppi Triangle", I decided to do some detecti ve work. Then I found in Marshak's article in the book, "The Birth of Particle Physics", (edited by L.M. Brown and L. Hoddeson), the Fig. 23.3, with the caption: "The Puppi Triangle ... From G. Puppi in Nuovo Cim. 5, 587 (1948)". Actually, this figure does not exist in Puppi's paper, even in the extended version (Nuovo Cim. 6, 194, May 1949). Instead this triangle appeared in print first in our R.M.P. paper. Marshak told me that he used the then current term "Puppi" triangle without further checking, in a short section of a long paper. Also in the same volume, covering the 1980 Fermilab symposium, Conversi refers only to Pontecorvo, Klein and Puppi papers. Who started to circulate that interpretation (that I shall show is incorrect)?

In some references, the Clementel-Puppi paper (Nuovo Cim. 5, 505, received Aug. 21, 1948), which has nothing to do with Fermi Interaction, is included as such. This is explicitly done in the Chronology of Particle Physics, by J. Six and X. Artru, presented in the 1982 Paris Conference. In Okun's book, "Leptons and Quarks" (1982), the Clementel-Puppi paper is attributed to Puppi. The other two references given by Okun on the hypothesis of universal weak interactions, are Pontecorvo (Phys. Rev. 72, 246, 1947) and Klein. None of these papers made such an explicit hypothesis. The C-P paper dealt with spinless (μ-μ_o) as Lodge had done (Nature, 161, 809, 1948).

Let us consider first the paper of Puppi (a short letter published one month before our R.M.P. papers but submitted much later than ours). There Puppi deals first with the μ-capture via $\pi\mu\mu_o$ direct scalar coupling (g_μ) and π-nucleons scalar coupling (g_p), adjusting the rate of μ-capture to the experimen tal value, thus obtaining the correct order of magnitude for the π-lifetime. Thus he used two Yukawa interactions, one strong and one weak, not a Fermi interaction. This calculation had been done before by Marti and Prentki (J.dePhys.9, 147, 1948), and by Leite Lopes (Phys.Rev. 74, 1722, 1948).

Next, using the explicit hypothesis of a Fermi interaction coupling (G) of $(\mu\mu_o)$ with $(e\nu)$, he obtains the lifetime for μ-decay, which gave $G \sim G_F$ (for β-decay). In this paper Puppi does not refer to the results on μ-decay $(G \sim G_F)$ published by Klein in the June 5, 1948 issue of Nature.

Finally, in a last short sentence, without any previous mention or motivation to consider the possibility of a Fermi interaction among (n,p) and $(\mu\mu_o)$, Puppi states that "the Fermi constant of the μ-capture process $(4\pi g_p g_\mu / k^2_\mu \hbar c)$", $G_F^2/\hbar c$ and $G^2/\hbar c$ result approximately equal. (Do not mind the slip of language in the comparison of the μ-capture Fermi constant with the square of the other Fermi constants). If Puppi had used, instead of the scalar, the pseudoscalar theory for μ-mesons, as later done by Leite Lopes (Phys.Rev. 109, 509, 1957), he would have found an effective Fermi constant for μ-capture 10 times larger than G_F, which shows that his near equality was a coincidence. Besides, the insistence on the F.I.mediated only by the π-meson with direct weak interaction with $(\mu\mu_o)$ is known to be unsatisfactory from the work of Goldberger-Treiman which shows that, on the contrary, the π-$\mu\mu_o$ interaction which leads to π-decay is induced by the Fermi μ-capture interaction. Credit must be given, however, to Puppi, Marti-Prentki and Leite Lopes for the prediction of the (now pseudoscalar) π-induced Fermi-type interaction $(p\ n\ \mu\ \nu')$. Only in the abstract of the more extended paper

(Nuovo Cim. 6, 94, May 1949 - received March 14) does Puppi state explicitly that "existence is found of a Fermi interaction between... nucleons, μ-mesons-electrons which envolve the same interaction constant. Of only one... precisely of the nucleon--μ-meson one, it exists also the corresponding description by means of a field (π-mesons)". Indeed the text has the same structure of the Letter to the Editor and, contrary to β and μ decays where the direct (point) Fermi interaction is used, in μ capture the Yukawa interaction for the involved fermions is used (scalar π), not the direct Fermi interaction. Here the effective Fermi coupling $G_{P\mu}$ is not calculated directly from Fermi μ-capture from but π-nucleon scalar Yukawa coupling and a π-μ decay direct coupling.

Potencorvo's paper (Phys.Rev. 72, 246, 1947), which I did not know at the time of my collaboration with Wheeler, showed, prior to the discovery of π and μ mesons, that if the cosmic--ray meson ($M^{\pm}$) had spin 1/2 and if, besides the strong nuclear interaction, it has also an ordinary Fermi interaction (with μ in place of e), then the results of Conversi et al. experiment are obtained if only the Fermi interaction works in the μ nucleon capture. Thus the production of a single $M^{\pm}$ would be unlike only if the strong coupling of M'S with nucleons were in $M^{\pm}$ pairs (associated production), as in the 1940 Marshak theory, or in multiple pairs which would agree with Heisenberg's multiple production theory. It is possible that it was the wrong interaction properties of the sea level mesons that made Pontecorvo's paper somehow ignored at the time. It is not clear why, after knowing of the π-μ discovery, Pontecorvo did not make a reformulation of this paper identifying the sea level mesons with the μ's and correcting his predictions for $\mu \to e + \gamma$. In any case he explicitly stated the significance of the analogy (via Fermi direct coupling) of μ-capture and β-capture, for which he should have credit.

In Klein's paper in Nature, a view opposed to Fermi's is examined. π mesons and nucleons were considered to be composed of μ's and μ_o's. The $\mu \rightarrow \mu_o + e + \nu$ decay was assumed to be the fundamental weak process (via an Yukawa "electro-photon" field). At the end of this extensive "note", Klein states that if $\sigma \equiv \pi$ (as it happens to be), almost all previous considerations would not be correct. However he stated, the interpretation of spin 1/2 μ-mesons with an ordinary β-decay (explicit Fermi interaction) would stand, having a better foundation. For this sentence and the corresponding calculations, a very small part of the paper, Klein should have credit.

Summarizing, I would like to make the following statements.

1. Only Tiomno-Wheeler and Lee-Rosenbluth-Yang papers examined both μ-decay and μ-capture by direct Fermi interaction showing the near equality of the couplings with that of nuclear β decay: $G_d \sim G_c \sim G_F$.

2. Klein and Puppi showed that in μ-decay, $G_d \sim G_F$.

3. Pontecorvo showed that in μ capture, $G_c \sim G_F$.

4. Marty-Prentki, Leite Lopes calculated μ-capture via scalar π-mesons finding that it was of the order of the experimental result. Puppi made the equivalent statement that the "effective" Fermi coupling via scalar π's is of the order of G_F. Leite Lopes corrected later these results using pseudoscalar π-mesons.

5. Only Tiomno-Wheeler's analysis was extensive, conside

ring several forms of Fermi couplings and several possible μ_o masses as well as several models for nuclear capture with account being taken of nuclear excitations and spectrum of emitted neutrons to obtain results to be compared with experiment.

6. Tiomno-Wheeler results indicating universitaly of Fermi interaction were presented at the Caltech Symposium (June 23, 1948), sent for publication in September and published in the January 1949 issue of Review of Modern Physics, which constitute the proceedings of the symposium.

I am thankful to Professor Robert E.Marshak for many helpful criticisms and to Professor Leon Lederman for the hospitality at the Fermilab where this paper was written.

ADDENDUM

Professor Marshak called my attention to the fact that I could have made the correct choice V - A instead of S+P-T already in my 1957 paper if I had used Ruderman-Finkelstein's prediction[i]: $R = \pi_{e2}/\pi_{\mu 2} \sim 1$ for Pseudoscalar interaction, $R \sim 10^{-4}$ for A and $R = 0$ for S,V or T weak interaction. It is ironical to mention that, already in 1950, my wife (EFP) had proved[ii], using nuclear emulsions, that $R < 5\text{x}10^{-3}$ which excluded P and thus the alternative S+P-T therein preferred for the parity violating U.F.I.

---

i - M.Ruderman and R.Finkelstein, Phys.Rev.76, 1458 (1949).

ii - Elisa Frota-Pessôa and Neusa Margem, Anais Acad.Brasil.Ciências, 22, 371 (1950).